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THE JOURNAL OF PHILOSOPHY

A BEHAVIORISTIC ACCOUNT OF THE LOGICAL FUNCTION OF UNIVERSALS. I

PHYSICAL SIGNS AND SOCIAL SIGNS

THE aim of this essay is to explain the universal reference relation which language symbols bear to their objects. The function of a symbol is to determine a reference to a referent distinct from itself, such that this reference becomes a control over subsequent overt behavior to the symbolized object. What constitutes the reference is a thought or idea of the referent. In calling ideas references I am adopting the terminology of Ogden and Richards.[1] The relation between a symbol and its referent is the thought of the object as determined by the symbol.

It appears to us that most efforts to describe the kind of responses that may constitute ideas fail to state what are the functional properties of thoughts. This failure has led some to identify thoughts with symbols *per se*, and others to identify thoughts with any response to a referent initiated by a stimulus. The first can not account for the reference of an idea. The latter can not account for the stimulus function of thoughts.

I suggest that functionally stated an idea is a reference from (1) a symbol, its cause, to (2) a referent, and (3) is a stimulus to the subsequent overt response of the organism to the object thought of. Such writers as Holt, Mursell, and Wells fail to set up any criteria by which one may distinguish between ideas and non-conceptual responses. In like manner, it is this functional property of an idea which Ogden and Richards do not consider. The symbol observer's subsequent response to the symbol's referent is, then, a function, not of the symbol, but of the ideas or conceptual relation determined by the symbol. Mr. Holt, in making the overt response a "constant function" of the object itself, offers no function for ideas.[2] Nowhere do these writers suggest that the problem of symbols is any other than that of pointing out that a part of a "context" comes to call out a response similar to that called out by the

[1] Ogden, C. K., and Richards, I. A., *The Meaning of Meaning*, p. 11. Harcourt, Brace & Co., New York, 1930, 3rd ed., revised.

[2] Holt, E. B., *Freudian Wish*, pp. 165 ff. Henry Holt & Co., New York, 1915.

original context or object. What is obvious in their account is that if the response to the original object is not an idea, and if the response initiated by a sign or substitute stimulus is merely similar to the original response, then there is no differential trait which converts the response in the second case into a thought of the object for which the sign stands.

In holding that the function of communicative symbols is that of stimulating ideas and that ideas are substitute stimuli for the referents of symbols, I differ of course from Watson and Mursell, who identify ideas with lingual responses themselves. Mursell is quite specific. We "take knowledge to consist of speech reactions. . . . We do not have knowledge on the one hand and speech on the other. . . . We have nothing but speech."[3]

Hollingworth is more consistent in identifying ideas with signs. Watson and Mursell identify ideas with signs, but in order to hold that ideas are a species of behavior they restrict thoughts to lingual signs. However, if a thought of an object is the sign of an object, I see no reason for supposing that thoughts of objects are properties of an organism. We should say with Hollingworth that the bell sound is the idea of the food object "in so far as the bell sound leads to a salivary secretion appropriate to" the food object. A photograph of "a loved one" which "instigates words, acts, or feelings established in connection" with the loved one is a symbol and "hence an idea or thought of the absent person."[4] A map is an idea of a road, in so far as it is a symbol of a road. True, in order to be an idea, a sign must determine a response of its observer appropriate to a referent, but there is no necessity that such an idea must be constituted by the sign observer's reactions. It may seem quite strange that one's thought of a kiln may be constituted by a brickbat rather than by some unique behavior of one's own. But this conclusion is absolutely valid if ideas are identified with symbols of referents.

A sign, as Hollingworth suggests, does refer to a referent, an object distinct from and beyond the sign itself. But what constitutes this reference, the meaning relation between the sign and an object? What goes from the bell to the food beyond? Plainly it is the sign's function, i.e., the overt response which the sign initiates and its referent terminates. The only thing that transcends the sign is the response to something else which the sign initiates. Any overt response to an object as initiated by some phase of an object has then one property of thought, viz., that of being a reference to

[3] Mursell, James L., this JOURNAL, Vol. XIX (1922), p. 182.

[4] Hollingworth, H. L., *Psychology, Its Facts and Principles*, p. 135. D. Appleton & Co., New York, 1928.

an object distinct from immediate sensible stimuli. On the other hand the overt response to a referent is not functionally an idea-response, because it is not the stimulus to the sign-observer which may control his subsequent behavior to the sign's referent. Not until the response references to objects become delayed and function as controls over what the sign-observer will subsequently do, do the references of signs become ideas and signs become symbols.

Hence, to give a behavioristic interpretation of the reference relation of the language symbols, it is necessary to show how responses to objects intervene between the symbol and its referent so as to control the subsequent overt response to the symbolized object. Both the idea-responses and the subsequent overt behavior are directed toward the symbolized referent, but they are functionally different. A response functioning as a directing idea is not carried out.

I do not deny that communicative symbols are substitute stimuli. But I do contend that the responses to referents when evoked by symbols are themselves substitute stimuli for subsequent behavior to the objects symbolized. It is this *double* substitution which Watson seems to have overlooked.

The following pages will describe the manner in which symbols and idea-responses arise from those non-symbolic stimuli of gestures which lead directly to overt responses. In this I shall follow in general Mead's analysis of the social act.[5]

It is not possible to deny the existence of mind or consciousness or mental phenomena, nor is it desirable to do so; but it is possible to account for them or deal with them in behavioristic terms which are precisely similar to those which Watson employs in dealing with non-mental psychological phenomena (phenomena which, according to his definition of the field of psychology, are all the psychological phenomena there are). Mental behavior is not reducible to non-mental behavior. But mental behavior or phenomena can be explained in terms of non-mental behavior or phenomena, as arising out of, and as resulting from complications in, the latter.

I depart from Mead on two basic points: He held that the relation of meaning exists only in an overt social act, whereas I find meaning present even in non-social behavior. Secondly, he attempted to explain language on the basis of individual gestures,[6] whereas I am forced to conclude that linguistic symbols are by nature the common property of a group, and that it is for this reason that they can function as universals.

As already suggested, signs and symbols are functionally different kinds of stimuli. Hence, an analysis of signs and their re-

[5] Mead, George H., *Mind, Self and Society*, pp. 10–11. University of Chicago Press, Chicago, 1934.

[6] See below, pp. 543–547, in the second part of this article.

lation to referents is necessary to the investigation of symbols. There are two kinds of signs, physical and social. Their difference rests upon the fact that there exist structurally two different kinds of acts. One is physical or non-social. The other is a social act. A social act will be discussed later. A physical response is one whose occurrence is in no wise determined by the response of another organism. For example, the rat's eating cheese is a non-social act because it is controlled by the cheese. Sitting is a physical act in so far as its execution is controlled by the chair and not by a social stimulus such as a host's invitation to be seated.

Wherever the complete response involves the serial functioning of different structures such as locomotion, manipulation, and consummation, the object can not be identified as any particular receptor quality. Instead, the whole physical act (including its objects) consists of a *series* of functionally different stimuli and responses. Thus, the distance receptor phases (visual, olfactory, etc.) of the cheese control the locomotor phase of the eating response with respect to the terminal, tactual, and gustatory phase of the act of getting food. That phase of an object which terminates a response may be called the referent of the distance phase. Any phase of an object (visual cheese) which initiates a response terminating at a referent phase of an object may be called a sign stimulus. The meaning relation or reference which a sign has to a referent is defined by the response which it initiates toward its referent. It is commonly recognized that though a response is produced by a stimulus it is not directed toward it. The response is not complete until it finds its object or objective. Thus Mr. Holt, after giving his definition of behavior, says rightly that the "immediate stimulus" need not be "the object of which the behavior is a constant function . . .; the development from reflex action to highly organized behavior is one in which the correlation between stimulus and organism becomes less and less direct, while that between the organism and the object [here called the referent] of response becomes more and more prominent."[7] Again, Wells says, "The response is released by a stimulus [sign] and is to an object [referent] removed from the stimulus, except in rare and simple cases where the stimulus and the object coincide, as in the case of the protozoa."[8] Dewey expresses the same point when he says that the "response is not merely to the stimulus; it is, so to speak, *into* it."[9] In his example,

[7] Holt, E. B., *Freudian Wish*, p. 169.

[8] Wells, Raymond Wesley, "Behaviorism and the Definition of Words," *Monist*, Vol. XXIX (1919), pp. 135–136.

[9] Dewey, John, *Philosophy and Civilization*, p. 236. Minton, Balch & Co., New York, 1931.

seeing the light of the candle evokes a reaching response terminating in feeling the heat of the candle.

By a sign, then, is simply meant a stimulus controlling an overt response or terminal act affecting directly some other part of the environment. The response itself is the "objective reference" to a referent. There is nothing necessarily mental or conceptual about the relation of meaning. For a sign to exist it is only necessary that a stimulus call out a response directed to an object distinct from itself. For example, after association of a "dinner call" with the food object, the call will initiate the dog's going to the terminal food object just as did the original visual phase (sign) of the food object. But the meaning relation of the sign to its referent is still the response it initiates.

This account of signs and the relation of meaning has certain consequences. First, the fact that a sign determines a response that is an "objective reference" to another object gives no evidence that such a referential act is an *idea,* i.e., a thought. To be an idea or "a mental event" a sign's reference must be itself a stimulus to subsequent behavior to symbolized objects. A sign does not imply that an organism has an idea of the object. His response to the sign's referent constitutes his idea of the object only if, instead of being overtly executed, it functions indirectly as a stimulus to the kind of a response which he might carry out in the future. Even Russell, who interprets ideas in terms of images, asserts that signs may be of two sorts: those involving ideas and those which simply stimulate an immediate overt response to some object.

> The operation of signs may or may not be accompanied by conciousness. If a sensible stimulus A calls up an *image* of B, and we act with reference to B, we have what may be called consciousness of B. But habit may enable us to act in a manner appropriate to B as soon as A appears, without having an image of B as soon as A appears. In that case, although A operates as a sign, it operates without the help of consciousness. Broadly speaking, a familiar sign tends to operate directly in this manner, and the intervention of consciousness marks an imperfectly established habit.[10]

We indicate this distinction of function by the terms "signs" and "symbols."

If it be true that sign stimuli may control responses to referents directly responded to in the past, then we can hardly accept the account of those who identify signs with symbols and sign references with ideas. Such, for example, is the view of Wells. "A word [symbol] is a stimulus that releases a response that is the function of the denotation [referent] of the word." For example,

[10] Russell, B., *The Analysis of Mind,* pp. 197 ff. The Macmillan Co., New York, 1921. Cf. also "The Meaning of Meaning," *Mind,* Vol. XXIX (1920), pp. 398 ff.

a blow stimulates flight. If the word "blow" stimulates flight, then the original blow is the denotation of the word, and the flight is the reference of the word to its object. "It is the organic response which establishes the relation of reference of a word to its object." [11]

All that occurs here is that one sign has been substituted for another. There is no suggestion that the word stimulates a response that would become a blow if completed, but that, instead of being carried out, becomes a control over some subsequent overt response which might or might not be flight from the object for which the word "stands." Such a response, which involves an idea of what the sign stands for, is qualitatively and functionally different from a simple and direct response to a sign.

Ogden and Richards likewise assume that all acts of reference are thoughts of objects for which signs stand. "The peculiarity of all interpretation," they say, is "that when a context [object] has affected us in the past, the recurrence of merely a part of the context will cause us to *re-act* in the way in which we reacted before." [12] The part of the "context" is called a sign. The "reaction" evoked by the sign to the old object is called a "thought" or "reference" to the object. But a reaction evoked by a sign is not functionally different from that caused by the object itself. A sign is simply a substitute stimulus, but there is no suggestion that a sign (symbol) is a unique substitute which in some way enables the reactions to past objects to function as stimuli to the beginning of some new type of response to the object symbolized. In other words, the overt behavior of the organism, in their view, can not itself be the function of the idea or reference relation. Their theory accounts for the causality of signs, but gives no function to ideas.

The meaning or reference relation between a sign and its referent is indeed the "specific response" called out by the sign. But there is no reason to suppose that the meaning relation is equivalent to a conceptual relation. To be an idea, a response must not only be a reference to an object, but must also be a stimulus redirecting subsequent behavior to the object thought about. Nor do the proponents of the views criticized suggest any method by which a sign may become a symbol and idea-responses arise from overt responses. Substitution simply describes how signs are multiplied. It provides no suggestion as to how a stimulus evoking ideas may arise.

In so far as the response is controlled by a physical object, no phase of the response itself can be a sign to an organism. In the

[11] *Op. cit.*, p. 133.

[12] *The Meaning of Meaning*, p. 140.

physical or non-social situation an organism's response to the physical object is never the function of the sign's reference to its referent. The visual ball may evoke a throwing response to the tangible ball. But the throwing itself in no way controls the organism's throwing behavior. In other words, the observer's receptors of a physical sign are never affected by the meaning relation determined by signs, but by the stimulating physical object. The response itself which a mere physical sign determines never becomes the stimulus object controlling the subsequent behavior of any organism.

But this is not true in the case of social acts. Here we find a sign's meaning relation to be functionally different from that of physical signs. It was Mead's chief contribution to this analysis to show that a social act is the type of conduct out of which symbols and ideas may arise.

> What is the basic mechanism whereby the social process goes on? It is the mechanism of gesture, which makes possible the appropriate responses to one another's behavior of the different individual organisms involved in the social process. Within any given social act, an adjustment is effected, by means of gestures, of the actions of one organism involved to the actions of another; the gestures are movements of the first organism which act as specific stimuli calling forth the (socially) appropriate responses of the second organism. The field of the operation of gestures is the field within which the rise and development of human intelligence has taken place through the process of the symbolization of experience which gestures—especially vocal gestures—have made possible. The specialization of the human animal within this field of the gesture has been responsible, ultimately, for the origin and growth of present human society and knowledge, with all the control over nature and over the human environment which science makes possible.[13]

An overt social act considered as a whole consists of the co-determining responses of two or more organisms. A dog fight, boxing bout, buying and selling, attack and defense, giving and taking, are examples of overt social acts. A social act does not consist of a summation of individual acts that might exist independently of each other. The unique feature of a social act is that the response of each organism can exist only as the directly determined occurrence of the response of another organism. The completion of the attack which one dog begins depends upon the type of defense offered by the other dog.

But what is the basic stimulus mechanism by which a social act goes on? According to Mead, this mechanism consists of gestures (language signs).[14] A gesture is any early phase of the act of one organism which stimulates a response of another organism directed toward the later phase of the gesturing act and is a stimulus

[13] *Mind, Self and Society*, pp. 13–14, footnote.

[14] *Ibid.*, pp. 53, 46.

to the subsequent response of the gesturer. Let us examine several examples. Let A and B be carrying out an act of throwing and catching ball, where A is catcher and B is pitcher. Suppose an early phase of A's catching is getting in position, such as cupping his hands. As a sign this gesture phase of catching stimulates the response of the pitcher directed to a subsequent phase of the catching, viz., A's grabbing the ball. Again, the bared teeth of dog A are the initial phase of A's biting dog B. But the bared teeth stimulate a response (vicious growl and snap) of dog B directed toward the later attack of A, such that B's response stimulates a retreat on the part of A. Since the reference or meaning relation which a sign bears to its referent is defined as the response it initiates toward this referent, the meaning, i.e., the reference, relation of a gesture to its referent, is the response of another; and *what* a gesture means to *the other* is the subsequent response of the gesturer. A gesture, then, has a referent, or meaning, where meaning is considered as what is referred to rather than the referential act itself, only for the other organism and not for the gesturer.

The structure and function of a gesture may now be more precisely defined. A gesture is a sign phase of the social response of one organism which controls the response of a second organism to the subsequent response of the gesturer. The subsequent response of the gesturer is the referent or the meaning (in the sense of what is meant rather than of the act of meaning or referring) of the gesture, because it is the objective of the response of the second organism. The completed catch is the terminal object of the throw initiated by the gesture phase of the catch.

There is, therefore, a marked difference between the meaning relation of a physical sign and that of a gesture. The subsequent response of the gesturer is a constant function of his gesture's meaning which he immediately observes in the response of another organism. Thus, the catching is the constant function of the pitching initiated by early gesture phases of catching and directed to the terminal or referent phase of catching. The dog that bares his teeth finds himself observing his gesture's reference and this is a stimulus to his subsequent response. The gesture determines the object series of stimuli which controls the gesturer's subsequent behavior. It is therefore the characteristic of a gesture that its reference response controls the subsequent behavior of the gesturer.

But there is nothing necessarily common about a gesture sign. As a mechanism of a social act, a gesture need exist only with respect to the sensitivity of that organism whose response it controls. Thus, in the case of the dog-fight, O_1's bared teeth are a gesture to O_2, who side-steps. A great deal of human behavior functions as gesture

signs which are meaningful only to other observers. Such, for instance, are most of our "facial expressions," the clenched fist in combat, and even many shades of our speech. If a gesture were a common sign, it would also stimulate a response in the gesturing organism. Such a common sign is not essential to the performance of a social act. The existence of a social act only requires that each organism be sensitive to, i.e., an observer of, the gestures of others in order that these may control his own responses. Thus, what is the least essential, in fact ruinous, to carrying on a boxing bout is that each boxer observe his own gestures as these stimulate the appropriate phases of his opponent's blows. If one is constantly sensitive to one's own gesture signs, one's behavior, being conscious, will slow up, and will be ineffective where rapid social adjustments are necessary. Mead held that we have no evidence that the members of sub-human groups can, by means of their own gestures, stimulate in themselves the responses which they stimulate in others. Mead's own theory implied that the dog would have to be able to take the rôle of the other. Regardless of Mead's theory, evidence for the use of common gestures here would consist, for instance, in a dog's baring his teeth merely to frighten and not actually to fight another dog. Even children make gestures long before they "understand" them, that is, observe them as signs. They act socially before they use their gestures consciously as imitations of adults. The principle may be laid down, then, that each social organism is as such an observer of the gestures of others, but not necessarily of his own. The gesture response is the other's sign and not the gesturer's sign.

Lastly, a gesture as such has no common referent and no common reference. The referent of any gesture is the subsequent response of the gesturer, and its reference to this referent is the response of the gesture-observer, which is another organism. The bared teeth of dog *A* refer to his subsequent bite, and this gesture's reference is constituted by the response of the second dog. The early wind-up of the pitcher refers to the pitcher's subsequent throw in so far as it controls the catcher's response to the subsequent throw. The catcher's getting in position refers to his subsequent catch. In this respect a gesture differs radically from a communicative symbol which has the same reference relation for every organism.

A gesture and its reference, i.e., the response it controls as found in another organism, are different from a symbol and its idea. But still there are striking similarities between a symbol and a gesture. First, a gesture is distinct from the referential response which it controls, just as an idea of an object is distinct from the symbol which determines the idea. Secondly, it is the gesture's reference

response which controls the subsequent response of the gesturer, just as an idea may control one's subsequent behavior to the thought of an object.

The question now arises as to how gestures may be transformed into symbols. In an overt social act controlled by mere gestures, it is the immediate response of another, *B*, as determined by the gesture of *A*, which is the controlling object of *A*'s subsequent response. *A* has no idea of the object of his response, viz., the behavior of another which functions as a control over his reaction to *B* when *B* is not sensibly present. Thus, the object of each organism's response is the response of another organism, and these constitute a common social act.

Our main problem now is to describe how a gesture would have to function in order to stimulate, in two or more organisms, responses which constitute, according to the above criteria, ideas of a common referent. The common referent in this case is, as we shall see, the overt social act as a whole, and the idea is a logical universal because its symbol is socially universal.[15]

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AN EXAMINATION OF COHEN AND NAGEL'S REPLY TO MILL

THE discussion by M. R. Cohen and Ernest Nagel in the fourteenth chapter of their book *An Introduction to Logic and Scientific Method* has as one of its chief points of interest a criticism of the traditional distinction between induction and deduction. According to the authors, the so-called inductive inference is not, as inference, distinguishable from deduction, whose essence is the derivation of conclusions necessarily involved in premises.[1] They point out that the conclusion of such an inference is not adequately established merely on the basis of an examination of some of the instances of a class. There is always taken for granted the truth of a proposition which asserts that what characterizes the examined instances characterizes all the possible ones. The so-called inductive inference is thus shown to be one in which a conclusion is necessarily involved in premises, and, hence, really a case of deductive reasoning. A distinction between inferences is more properly made, they say, on the basis of whether or not the propositions involved deal with matters of fact. If they do deal with matters of fact, inferences are

[15] To be continued in the next issue, No. 20.

[1] *An Introduction to Logic and Scientific Method*, pp. 276, 278.

called probable; otherwise they are known as necessary. In the probable inference the question of the material truth of the propositions is, therefore, a relevant one, and it is in connection with establishing the material truth of the premises of such an inference that we may properly speak of induction.[2] It may be viewed, say the authors, as "the method by means of which the material truth of the premises is established."

I take the authors' position to be that every genuine case of generalization can be thrown into the form of a syllogism in which the major premise states that whatever characterizes the observed instances characterizes all the instances of the class, and that when this is done, we indicate that the important problem in a specific inquiry is that of establishing the truth of this major premise. If its truth is assured, and the cases are accurately observed to have a certain character, the truth of the conclusion necessarily involved in these premises is also assured. In other words, if we suppose that the truth of the conclusion of a generalization is challenged, this is a challenge of the truth of the major premise upon which it logically depends. The truth of the major is established, as I understand the authors, by methods called inductive.[3]

Apparently the term "induction" as pointing to methods by which is established the truth of the major premise of a generalization does not, in their view, indicate a process of inference. To what, then, does the term point? Induction is described in one place as being specifically concerned in determining to what extent the instances whose examination is the basis for generalization are fair or representative.[4] If there is assurance that the samples examined are fair, this, evidently, gives assurance of the truth of the major premise, a premise asserting that whatever characterizes the observed samples characterizes all possible ones. The term "induction," apparently, points not to inferential processes but to ways of selecting instances of a class, the use of which issues in the selection of instances that are representative. A given character being observed in the representative instances, it follows necessarily that this character is the property of all the possible instances of the class.

On such a view of induction we should be able to test the truth of any generalization by determining in what manner the examined instances have been selected, if we are familiar with methods of selection which result in the choice of fair samples. Is there one such method or are there several? Is random sampling the only

[2] *Ibid.*, p. 278.
[3] *Ibid.*, p. 278.
[4] *Ibid.*, p. 278.

method of selecting representative instances of a class? To this important question the authors give no definite answer in the present discussion. But there seems to be indicated by their treatment a study of methods of selecting instances of a class in different fields of inquiry, eventuating, perhaps, in formal descriptions of correct methods of procedure which would serve as canons of proof of the representativeness of instances, and hence, of the truth of the major premise from which is logically derived the conclusion of the generalization. But beyond a short discussion of random sampling as a technique for selecting instances of a class which is not adequately distinguished from what they call the random sampling of various consequences of a hypothesis, the authors do not consider this implication of their view of induction.

More important than these considerations is a difficulty that arises when we find the authors indicating another way of determining that examined instances are representative. We can be assured of the fairness of samples, they say, when we know that the class of which the instances are members is a homogeneous one in relevant respects.[5] The homogeneity of a class can never be certainly established, it is pointed out, but when it is highly probable, as it is in some cases, the instances of that class are known to be representative. And this knowledge, as we have seen, establishes the truth of the premise that whatever characterizes them, characterizes all. Here the important matter of determining whether observed instances in a case of generalization are representative is decided in a way other than that of noting their mode of selection. In this way a process of inference is clearly involved; we assure ourselves that observed instances are fair by relying on established knowledge concerning homogeneity of classes. If we confine our attention to the major premise of generalizations whose truth is established on the recognition that the instances observed are representative, the authors' position seems to be that this may be done by methods called inductive, i.e., by selecting the instances in certain ways, or by inference from previous knowledge concerning the homogeneity of the class of which the instances are members.

The validation of generalizations by reliance upon previous knowledge of the homogeneity of classes rather than upon correct methods of choosing instances for examination is the one which receives the greater emphasis in the authors' discussion in this chapter on induction. As a consequence, we receive the impression that their solution to the problem of how we arrive at dependable generalizations from an examination of only a portion of the possible instances of a class is that in such cases there is always previous

[5] *Ibid.*, p. 281.

knowledge that the generalization is true. This impression is borne out by the reply that the authors make to Mill's famous question: "Why is a single instance, in some cases, sufficient for a complete induction, while in others, myriads of concurring instances, without a single exception known or presumed, go such a very little way towards establishing an universal proposition?" In their view, if one inference issues in a highly probable conclusion and the other doesn't, it is because the major in those cases of generalization where only one instance is involved, is established, while in others, though many instances are examined, its truth is not assured. Although only a single instance is examined, it is known to be a representative one.

How is this knowledge obtained? In the light of their remarks about induction we should expect the authors' reply to this question to take the form of showing that the assurance rests on a mode of sampling which eventuates in such instances. They say, rather, that it rests on the knowledge that the examined instance is a member of a homogeneous class. This knowledge in some cases of inference enables us to generalize with assurance upon the examination of a single instance; the lack of this knowledge, with the consequent uncertainty as to the fairness of the samples, makes generalization from the examination of even a large number hazardous and untrustworthy.

Setting aside for the moment any question as to the meaning of the term "homogeneity," how does one arrive at the knowledge that a class is homogeneous in relevant respects. The authors do not tell us. They assert simply that we do have such knowledge respecting certain classes, and it assures us that the instances of these classes are representative. How this knowledge is attained, whether by induction or not, and if by induction, whether in the sense of the term as indicating modes of selecting instances, to these questions their discussion gives no answer.[6]

Obviously it is no reply to Mill's question, which he thought stated the problem of induction, to say that when we generalize with assurance upon the examination of a single instance, we are deducing our generalization from previous knowledge concerning the homogeneity, in respects relevant to the inquiry, of the class of which the instance is a member. The question is how we know that? This answer to a question which professedly states the problem of induction leaves the reader with the impression that our ability to

[6] Unless the recognition that a class is homogeneous is the same as saying that the instances are recognized to be representative. But in such an event the statement that a class is established as a homogeneous one by certain modes of selecting instances of it becomes quite incomprehensible.

generalize with assurance is explained by the fact that in such cases we know before we attempt it that the conclusion is true. If there is such a thing as generalizing from the examination of a single instance, certainly the authors do not explain it by telling us that it is valid only where there is knowledge of homogeneity, unless they tell us how this important knowledge is gained.

To the extent that they emphasize knowledge of the homogeneity of a class rather than modes of selecting instances as a condition for dependable generalization, they are in the position of demanding that generalizations be known to be true before they may be proved to be true. Their reply to Mill's question indicates the degree to which they do emphasize knowledge of homogeneity as an explanation of our power to generalize with assurance. But the meaning of this notion, of its companion term "representative" as applied to instances of a class, and, as well, the interpretation to be put on the expression "whatever is true of the examined instance is true of all," are matters that are by no means even as clear as we have assumed them to be in the preceding discussion.

These difficulties may be brought out by examining the analysis that the authors make of a case of inference taken from the field of chemistry.[7] A chemist examines an instance K of a new chemical compound, O, finds it to have a density A, and concludes that this is true of all instances. They use this example to illustrate the case in which a highly probable general proposition results from the observation of a single instance. Should we observe a single instance of the class of red-haired persons to be hot-tempered and attempt to conclude that all red-haired persons are bad-tempered, the general proposition that results has only a very slight probability, if it has any. In the authors' view, what makes legitimate the inference of the chemist is his knowledge that the class to which his examined instance belongs is a homogeneous one in relevant ways. He then knows that instance K is a representative one, and, hence, that whatever characterizes it will characterize all instances of the compound. It follows that they will all have the density A, if K has that density.

Now in what sense of the terms can the recognition that an instance K is "representative" guarantee the truth of the proposition asserting that *whatever* characterizes it characterizes all possible instances. In considering the chemist's grounds for believing that all the instances of his new compound will have the density A, the authors point out that it is based "upon the assumption that this sample of the compound is homogeneous with all other samples as

[7] *Ibid.*, p. 281.

far as their physical properties are concerned, and upon the general proposition that density is constant for all homogeneous substances."[8] This passage indicates that by characterizing a class as a homogeneous one in relevant respects, the authors mean that each possible instance of it will be found to be similar to every other in the possession of traits which are "relevant," in some sense of the word, to the inquiry at hand. When it is said, then, that a knowledge of such "homogeneity" assures us that any instance of the class is "representative," all that this term seems to mean, is that one instance is as good as another for purposes of examination, in that each is known to have the traits that every other has. In the case before us, these traits are certain physical properties. When we say, then, as the authors assure us we may, that whatever are the characters of one of the instances, or of two, or of three, are the characters, as well, of all, it is important to note that this assertion can not be interpreted as stating that *any* character that an instance is found to have all will have. The recognition that an instance is "representative," i.e., that it shares with all the possible instances of the class a given set of traits, certainly does not permit the assertion that *any* character this instance is observed to have, all will be found to possess.

The proposition asserting that whatever is a character of the observed instance is a character of all of them is, therefore, not established on the recognition that an instance is "representative" if by "whatever" is meant "any." Unless, however, we interpret "whatever" as meaning "any" the proposition can not serve as the major premise in a case of generalization. To be sure, if the proposition is so interpreted and is true, and the examined instance is correctly observed to have a certain character, then the conclusion which asserts that this trait is the property of all instances follows and is true. The "representative" character of an instance which the known "homogeneity" of the class enables one to assert does not, however, establish the truth of a major premise so interpreted. The traits which instances are known to share may be as "relevant" as one pleases, but this does not alter the case. The "relevance" of certain traits that the instances share to the inquiry as to whether they also share some further trait observed in a single instance does not help to establish the assertion that *any* character not known previous to the inquiry to be shared by all is actually a trait of all.

The knowledge that a class is a "homogeneous" one in "relevant" ways with its assurance that an instance of that class is "representative," does not, in the only meaning that I can discover in the

[8] *Ibid.*, p. 281.

authors' use of these terms, establish the truth of a proposition asserting that whatever is true of the examined instances is true of all possible ones, if this is interpreted as meaning any trait that the examined instance has is the property of all. Such a position would enable one to assert that all the gold in the world is in the possession of Ethiopia because some instances of it are, these instances being "representative" in that they are members of a "homogeneous" class, i.e., are known to share with all possible instances a certain set of traits. However the "relevance" of traits is determined, this would not add to the weight of the evidence.

If we state the proposition that is true when the examined instance *K* is recognized as one of a "homogeneous" class and hence "representative," it can only be one which asserts that whatever traits the examined instance *K* shares with all others (in this case certain physical properties) are traits that will be found in all the instances. But this statement, as true as it is that *A* is *A*, can not serve as a premise for a conclusion asserting that all the instances have some further trait observed in the single instance *K*. If we know, for example, that all the instances of a class possess the properties *m*, *n*, and *o*, and, further, that the "homogeneity" of the class in these respects is "relevant" to an inquiry into the truth of the proposition that all the instances have another property *p*, let us admit with the authors that a given instance is "representative." This is not an admission difficult to make because, as asserting that the given instance is like all others in the possession of properties *m*, *n*, and *o*, it is the equivalent of the previously granted homogeneity of the class in respect to these properties. This knowledge certainly contributes nothing in the way of evidence for the truth of the general proposition that all of them possess the further property *p* that we observe in the single "representative" instance. This proposition is not logically dependent upon such knowledge.

In short, the recognition that an examined instance is a "representative" one, a member of a class "homogeneous" in "relevant" respects, in the authors' use of these terms, does not establish the truth of the proposition upon which the conclusion of the so-called inductive inference is said by them to depend logically. And the proposition that can be asserted as true on the recognition of the examined instance as "representative" is not one upon which such conclusions can be said to be logically dependent.

The analysis which the authors themselves make of the chemist's inference indicates that what they assert to be the sole condition of valid generalization from a single instance is not of itself sufficient. For they point out that the ground of his inference is not only that

his subject-matter is known to be homogeneous as far as physical properties are concerned, but also that a subject-matter which is homogeneous in these respects is known to have a constant density. The mere knowledge that the subject-matter is homogeneous in relevant respects is not sufficient even on the authors' position. One must know something else to be true; one must know that its homogeneity in relevant ways points to the presence of further traits present in all members of the class.

The trouble seems to me to arise from the vagueness of the term "homogeneous." In the field of chemical inquiry when we have something we can call a chemical compound, we can be sure that we have what is called a homogeneous substance. What does this mean? It means that every particle of it has certain properties and that these properties appear in the same measure in every instance of the substance, given certain specified conditions. This is what it *means* to be a homogeneous substance. The chemist, knowing all this, identifies what is before him as an instance of such a substance, and without hesitation affirms the discovered traits of his new compound. One of these is the trait of having a certain density. He does not verify the general proposition concerning the constancy of the density; he accepts it as sufficiently established. He knows that if the single instance of the compound before him has a density of a particular value, they will all have the same one. His only inquiry is *what* particular value of density is constant in all the instances of the compound.

If the authors have presented the chemist's inference as a case in which the known homogeneity of the subject-matter in relevant respects operates as a condition for generalization from a single instance, there are two errors committed. There is the error of mistaking what the chemist does in this inference as a genuine case of generalization. What he does is give to a general proposition known to be true of the subject-matter a greater precision of statement. The situation is very much like that which obtains when we know that there are more than one hundred marbles in a box and make the statement more precise by counting one hundred and two. This error alone would be sufficient to throw doubt on the whole position, for the analysis of such a case would not exhibit the conditions of legitimate generalization from a single instance.

The second error is related to the first and perhaps accounted for by it. It is the failure to see that the term "homogeneous" as it is used in the illustration takes on the meaning that it has in a special field of inquiry, a sense which is different from the one which points to conditions of inference in any field of inquiry. If the term "homogeneity" is to state the condition for inference in any

case where a general proposition is established by the examination of a single instance, the special meaning that the term has in a given field can not be adequate, unless the authors mean that we can so generalize only when the subject-matter is "homogeneous" in this special sense. In this event it would prevent confusion were they to say that inferences of this sort can take place only in the fields where homogeneous substances, in the sense in which the chemist uses this term, are the subject of inquiry. Such is not the meaning that I take from the authors' discussion. The condition of generalization is stated to be that of knowing that the subject-matter is homogeneous in relevant ways, which, as we have seen, is simply the demand that the instances be known to be alike in the possession of traits relevant to the inquiry in hand. This is not the demand that the instances be alike in certain physical and chemical properties. It just happens that for the chemist's inquiry these are the relevant respects in which they are alike; their likeness in these respects may have been completely irrelevant.

The authors have confused their meaning of homogeneity as defining the logical requirements of any generalization with the meaning that the term has when a homogeneous substance is spoken of. Thus when in the illustration they have thought themselves to be pointing out the way in which homogeneity in the logical sense operates in inference, it is only homogeneity in the chemist's sense that lends to their analysis of the grounds of his inference its apparent plausibility.

We get no clear notion of the application of the term "induction" from the discussion in this chapter. It points, apparently, not to a process of inference but to methods of selecting instances of a class which eventuate in representative samples, whereby we are assured of the truth of a major premise upon which all so-called inductive inferences logically depend. But in replying to Mill's crucial question, which for him, at any rate, stated the problem of induction, the authors seem to me to desert this position. They contend that we may generalize with assurance on the basis of an examination of a limited number of instances of a class when there is knowledge of what they call the homogeneity in relevant respects of that class. They do not, as we have seen, tell us how this knowledge is to be gained. Their answer to Mill is, in substance, that one may generalize with assurance on the basis of the examination of a single instance when one has previous knowledge from which it may be logically derived. This knowledge of homogeneity rather than modes of selecting instances becomes the condition validating generalization. We have seen that there is good reason for believing that the authors take this as the solution of the problem of induction,

since this notion of homogeneity dominates their discussion and it is in terms of it that they make their reply to Mill's question.[9]

Aside from the difficulty of accepting known homogeneity of classes as the explanation of how we can generalize from some to all, troubling confusions arise out of the ambiguous use of this idea of homogeneity. When it is carefully analyzed and applied, knowledge of it is shown to be insufficient to establish the truth of the premise upon which they say the conclusion of the inductive inference logically depends. If, moreover, the premise which they offer is interpreted in such a way as to be true when one instance is known to be "representative," it is not a premise upon which such a conclusion can be said to be logically dependent. The premise becomes simply the equivalent statement of the fact that the instance is "representative," true if this is admitted, but leading to no generalization about a further trait observed in that instance.

It is the ambiguous use of the term "homogeneity" which, I am persuaded, lends the appearance of plausibility to the authors' treatment of induction. If the foregoing criticism is a just one, the discussion does not exhibit, as it claims, the conditions of generalization from the examination of a limited number of instances.

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[9] Though they distinguish between early and late stages in the development of a science, the authors seemingly accept Mill's question as an adequate formulation of the problem of induction. R. M. Eaton, in his *General Logic*, pp. 484, 486, disposes of the question by suggesting that it arises out of a conception of induction which fails to distinguish between what he calls a primary and a secondary phase. The puzzle is immediately cleared up for him with the acceptance of this distinction. The answer that we can give to Mill's question on the basis of the distinction, however, leaves the problem of induction still unsolved, namely, that of finding a way to show "that the general conditions of some system are fulfilled by a set of particulars assumed to belong to the system, when it is not known that these are all the particulars in the system." This treatment of Mill's problem is, in my opinion, clearer and more suggestive than that of the authors'.

BOOK REVIEWS

Meister Eckhart. Käte Oltmanns. (Philosophische Abhandlungen, Band II.) Frankfort: Vittorio Klostermann. 1935. 213 pp. 6.50 M.

This book of Miss Oltmanns' brings a new note into the ever-growing literature on Meister Eckhart. She attempts in a new way to solve the riddle of the relation of Meister Eckhart's scholasticism and

mysticism and to find the true meaning of this mysticism. Her method is comparable to that used with such success in the study of ancient philosophy: to form a criterion of the attitude and intention of Meister Eckhart from his indubitably genuine work and to judge by this criterion the authenticity of the attributed work. This method would in itself be admirable if used with the philosophical accuracy and rigor which led to such successful results in the investigation of ancient writers. Miss Oltmanns seems far from such cautious accuracy. She merely enumerates the writings that on the basis of her method she believes to be spurious without giving her reader the special reasons for her choice. Thus she eliminates a great number of the very writings that others have made the foundation of their estimate of Meister Eckhart's work. The presentation that Miss Oltmanns gives of Meister Eckhart is itself interesting and suggestive—it may even be true—but as long as she gives her reader her unsubstantiated intuition alone as a basis for her attributions to Meister Eckhart, her work is open to attacks from all sides.

M. A. G.

Le réalisme méthodique. ETIENNE GILSON. Paris: Pierre Téqui. 101 pp. 12 fr.

Few books of so slight bulk contain as much nutriment as these four essays by M. Gilson. Though they were originally published as separate articles, they have a common theme—the necessity of basing epistemology upon metaphysics rather than of deducing a metaphysics from epistemology. Methodical realism, if that is a satisfactory translation of *le réalisme méthodique,* is fundamentally the realism of St. Thomas. To St. Thomas the problem of knowledge was not how to prove that one knew something not one's own ideas, for his philosophical method made it impossible for him to conceive of knowledge as a mosaic of ideas in the sense of mental states produced by physical causes. Idealism, as we all have reason to know, may become as "objective" as it will; reality is contrasted with appearance simply as the *consensus gentium* is contrasted with personal opinion. This is, to be sure, a crude way of stating the subtleties of post-Kantian philosophies, but in spite of verbal refinements, the sentiment remains the same. What M. Gilson does not bring out clearly enough is that the reason why St. Thomas was able to avoid the problem of the reality of the external world lay in the fundamental Aristotelian assumption of his epistemology: namely, that knowledge is the actualization of a potency. One who reads the *De anima* and the *Parva naturalia* carefully is bound to discover that to translate their theory into what Reid called Cartesian language is impossible. M. Gilson recognizes the sterility of Thomism

for science; what reason has he to think that we can adopt its basic metaphors in one field and abandon them in another? Can we fruitfully return to the potentiality-actuality formula and reach results applicable to what we know—or at least feel best to believe—the world to be to-day? The substitution of events for billiard balls does indeed lend some encouragement to the venture, but on the other hand the Aristotelian formula demanded a permanent substratum which modern physics would be hard put to it to provide.

This restriction aside, the reader of M. Gilson's book will probably agree with its publisher that reading it *procure une impression bienfaisante de santé et de liberté.*

G. B.

The Philosophy of Our Uncertainties. A Comment on the Uncertainties of our Philosophies. GUSTAV E. MUELLER. Norman, Oklahoma: The University of Oklahoma Press. 1936. Pp. xii, + 237. $3.00.

The attempt to base certainty on uncertainty, to find in the awareness of tension, opposition, and limitation the ground for assurance of a reality that transcends such limitation and in which opposites are harmoniously united, is philosophically a familiar one. Professor Mueller's enthusiastic and sometimes eloquent reaffirmation of the chief tenets of this type of dialectical idealism is notable chiefly for its literal fidelity to "the masters of the history of philosophy" who, as he observes, have said these things before, and for the pertinent criticisms which, from this standpoint, he is able to direct against the "one-sidedness" of many contemporary theories. To the orthodoxy of his dialectical method and conclusions the following passages will testify: "Real is what is thinkable. If this is true then the content of thought is not a 'stuff' or 'matter' around which forms of thought are fixed like frames, but these forms of thought penetrate and *are* the other-than-thought. Thought posits and realizes itself by othering itself and taking the expression of this seeming other as its own" (p. 89). "The absolute being is neither transcendent nor immanent, but is revealed as well as conceived as a unity othering itself and in its self-alienation nevertheless preserving its absolute, all-comprehensive nature" (p. 24). "The certainty of uncertainty is an immediate certainty mediated and brought about by thinking itself" (p. 100).

Though the terminology of this philosophy is at present somewhat out of fashion, the insight it expresses is still of major significance. Its emphasis on the need, for philosophy, of a theory that does justice to all aspects of experience is a valuable corrective of the negations of much current "analytic" philosophy. And it is

still useful to point out that those who adopt a standard of truth or validity as a sceptical or critical instrument should be prepared to assume the philosophical responsibilities to which the acceptance of that standard commits them. In its critical passages, therefore, Professor Mueller's dialectic is often valid and enlightening. But his constructive theory is, it seems to me, open to damaging objections. Briefly, the solution of philosophic problems by a dialectical resolution of opposing theories seems in this version at once too easy and too difficult a project. Given conflicting doctrines, each of which overstates some and neglects other phases of the situation it purports to describe, it is a comparatively simple matter to observe that the truth lies somewhere between the extremes of misinterpretation and is more concrete than any incomplete account of it. Thus, in epistemology, the opposite and equally "abstract" theories of "objectivism" and "subjectivism" point beyond themselves to a truth which will include their virtues and eliminate their defects. Yet this, in itself, tells us very little about the knowledge situation or the relation of knower to known. Professor Mueller's reconciliation of opposing theories seems to me frequently to be of this rather too easy sort. In another respect the proposed reconciliation is prohibitively difficult. If reality is dialectical, a real tension and conflict between opposites, then something of this tension must be preserved even in the unification or harmony which constitutes the absolute truth. But equally, if this opposition consists in a real contradiction, it is very hard to see how the unification in question can preserve it. Contradictories are unified, one would suppose, only by being shown to be not really contradictory. The unity sought seems thus to be obtainable only by a demonstration of the unreality of the opposition which led up to it; the harmonious reality has no place for the dialectic which is supposed to be of its essence. This difficulty is, of course, not new. But it appears to be fundamental. And I can not see that Professor Mueller has shown us at all clearly how his dialectical idealism would meet it.

A. E. M.

Truth and Corrigibility. HENRY H. PRICE. Oxford: At the Clarendon Press. New York: Oxford University Press. 1936. Pp. 31. 75c.

This is a reprint of an inaugural lecture delivered before Oxford University. It is a piece of dialectic which must have been something of a strain on the attention of its hearers. Under the head of corrigibilists, those who believe every judgment is an hypothesis subject to correction, the author brings together such supposedly diverse schools as the absolute idealists and the extreme

logical positivists, though omitting the more obvious reference to the pragmatists, and avoiding expressly the field of pure mathematics. It is an acute piece of reasoning, and advances from the fairly obvious, one step at a time, to conclusions that are novel, at least in emphasis.

H. T. C.

The Revolution in Physics. ERNST ZIMMER. Translated, and with a preface, by H. Stafford Hatfield. With an Introduction by Max Planck. New York: Harcourt, Brace & Company. 1936. xiv + 240 pp. $3.75.

There have been translated into English recently, from German, two rather readable and yet valuable books on the present situation in atomic physics, namely, *The World in Modern Science,* by Leopold Infeld, and *Atom and Cosmos,* by Hans Reichenbach. Max Born's *Restless Universe* is less useful to the layman, because it contains a good deal of the author's own speculations too briefly adumbrated. But Zimmer has given us a book nearly four years later than Reichenbach's, almost as easy to read, and of equal philosophical interest, though of narrower scope. For the philosophical reader these translations are probably more worth attention than anything recent and non-mathematical in English, though Millikan's book on *Electrons Plus and Minus* should not be overlooked, and there are other books of some size, such as the sixth edition of James Crowther's *Ions, Electrons, and Ionizing Radiations,* which give one a more adequate idea of the experimental evidences and how they are obtained.

Zimmer is a follower of Planck, and his book, especially the last chapter, gives a rather clearer presentation of this point of view than Planck himself has done. Zimmer is a critic of the scientific positivism coming down from Mach, which rejects all hypotheses that go beyond what is verifiable in some immediate experience or other. Zimmer objects that this line of argument logically leads to subjective idealism or even to solipsism, that its defenders seldom hold to it consistently in all they assert, and that when they do, it becomes sterile in suggestions for new experiments. The Bohr theory of the atom, on the contrary, went beyond the verifiable and finally could be definitely refuted, but it strongly stimulated inquiry while it lasted. The physicist should think of himself as building up a "physical world-picture," much of which is admittedly mental construction and only symbolic of the real situation. This picture possibly contains elements which are beyond present modes of verification, and baffling even to our established modes of thought.

But it must imply consequences capable of further observational verification, and be susceptible of indefinite modification to make it more and more approximate to the real nature of the physical world. The scientists' physical world-picture of the present moment forces us, indeed, to think in startlingly new ways, for it is not in agreement with our common-sense picture of the world. It is not even expressible in the words of ordinary language. Though in a sense a creation of the mind, its implications are independent of its creator, and carry the mind outside itself, as it were, forcing us to think in novel ways. Neither can we suppose it as yet in satisfactory agreement with objective fact. Every year brings notable changes. But nevertheless such a picture sustains the imagination, and suggests experiments whereby it may itself be transcended and transformed.

H. T. C.

La logique et l'empirisme intégral. JULIEN PACOTTE. (Exposé de Philosophie des Sciences, V.) (Actualités Scientifiques et Industrielles, 224.) Paris: Hermann & Cie. 1935. 56 pp. 12 fr.

Rejecting the classic metaphysical approach to logic and the aprioristic conception of knowledge and science, M. Pacotte expounds the contemporary views with respect to both which are dominated by the ideas of Boole, Poincaré, Duhem, Mach, and Bergson. He characterizes reality as that which is given to immediate experience as a qualitative continuum, freed from the systematic interpretations of it given in the sciences. Logic and mathematics, on the other hand, are taken as studying the necessary interrelations of abstract symbols, freed from every metaphysical or sensory interpretation. Hence the formal and the real or concrete are said to be radically heterogeneous, and the task of an adequate logic of science is to show how the correspondence between them is brought about. M. Pacotte briefly discusses modern mathematical logic and physics, and outlines the tentative procedure of scientific induction in search of abstract patterns to encompass the qualitatively heterogeneous reality. As to the metaphysical faith which actuates many scientists that nature is a system intelligible in terms of elementary mathematical laws, his comment is: "Il ne peut rien perdre à la ruine d'une métaphysique de l'intelligibilité, pourvu que l'on édifie la science de l'intelligibilité, ce qui est précisément la définition même de la logique."

E. N.

La prévision historique dans la nature. J. DELEVSKY. (Exposés de Philosophie des Sciences, VII.) (Actualités Scientifiques et Industrielles, 304.) Paris: Hermann & Cie. 1935. 53 pp. 12 fr.

Distinguishing between conditional predictability (in which an event is foretold subject to the realization of a number of hypotheses, and best illustrated in classical mechanics) and unconditional predictability (in which all the necessary conditions for the occurrence of an event are taken to be realized), M. Delevsky undertakes to show that historical predictions are of the latter kind and to inquire into the feasibility and extent of such predictions. He offers ample evidence that even in classical mechanics our knowledge is approximate, and gives an impressive catalogue of our ignorance of historical laws in astronomy, geology, climatology, seismology, biology, and medicine. The conclusion of this survey is that historical (or unconditional) predictions are only probable and approximate. The author argues that this arises in part from there being no absolutely identical elements in nature, in part from the ultimate particles of matter behaving by "pure chance" (a view which M. Delevsky regards as tenable but unproved), and in part from the interaction of human beings with their environment, who partly foretell the future only to alter it because they act on the basis of their own prophesies.

E. N.

La méthode dans la mécanique des quanta. (Axiomatique, déterminisme et représentations.) RENÉ DUGAS. (Exposé de Philosophie des Sciences, VI). (Actualités Scientifiques et Industrielles, 283.) Paris: Hermann & Cie. 1935. 61 pp. 12 fr.

The greater part of this brochure is devoted to a brief description of classical mechanics, the innovations introduced by relativity theory, and the evolution and central ideas of quantum mechanics. A final section contains a discussion of some methodological issues raised by these modern developments, especially with respect to determinism and the question whether physics to-day provides us with a picture of some substantial reality. In partial opposition to Meyerson's views, M. Dugas concludes: (1) that although quantum mechanics does not show the failure of classical determinism, the latter is applicable in all rigor only to certain types of highly selected phenomena, since a good deal of "cooking" of empirical material must in any case be performed and such intuitive pictures as waves and particles are introduced primarily to yield rigorous deterministic laws; and (2) that concepts like those of wave and

particle are not representative of some substantial reality, because they are in fact simply shorthand for certain mathematical functions. Quantum mechanics is thus viewed as a manifestation of pan-mathematics,—a logical or linguistic elaboration of certain empirical data, useful in the interest of continuity, but not a "picture" of "reality." In the main, therefore, it is to the methodological principles avowed by Dirac that M. Dugas himself subscribes.

E. N.

L'essor de la philosophie politique en XVI^e siècle. PIERRE MESNARD. Paris: Boivin & Cie. 1936. viii + 711 pp. 75 frs.

Having pledged fidelity to the French school of sociology in his *Avant-propos,* Dr. Mesnard cites Comte's conviction that prior to his time a positivistic sociology was impossible inasmuch as the conditions requisite for it had not yet been fulfilled, preliminary to announcing the thesis that prior to the sixteenth century the conditions requisite for a sane political philosophy had not been realized. Indeed a false dilemma still persists between political science which must be based on observation and philosophy in which theories are constructed without concrete elements. Political philosophy was impossible in Greece and in Rome because of the slightness of the experimental basis of ancient philosophers and because of their neglect of the experiences of neighboring states; in the Middle Ages too, writers on political theory were content to accept an integral, indisputable authority and to concern themselves with the problem of the abstract superiority of civil power or religious power. In the sixteenth century, however, intellectual, social, political, and economic circumstances conspired to set political problems—the nature of the state, the relation of states to each other, the source of political power—concretely before practical politicians and philosophers; to handle the abundance of material drawn from contemporary events and the history of the past it became necessary to consider the methods and concepts of political philosophy. Since Dr. Mesnard is convinced that the political and intellectual movements of the sixteenth century are so closely interwoven that no single strand can be understood in isolation of the others, he is at pains to set forth with detailed documentation not merely the outstanding examples of the numerous political theories which were developed in the century, but also their philosophical, metaphysical, and theological foundations and the social and economic currents in which those philosophic constructions arose. These interrelations are presented graphically in an appended chronological table and in detail in the successive chapters in which the political theories of Machiavelli, Erasmus, More, Luther, the Anabaptists, Calvin and

the Calvinists, La Boétie, Orzechowski, Modrzewski, Postel, Vitoria, Bodin, Mariana, Althusius, Suarez are presented. In the movements which extend from humanists, through reformers, legalists, theorists, and synthesists, Dr. Mesnard finds an immanent dialectic which produced three ideas characteristic of the century, the idea of the state, the idea of sovereignty, and the idea of the international community. Dr. Mesnard has amassed so large a collection of materials and has examined so many documents relative to the sixteenth century that it would be ungracious to criticize him for neglecting as much as he has the continuous history of political doctrines and for finding instead so much novelty in the terms and theories of the sixteenth century.

R. McK.

OTHER NEW BOOKS AND JOURNALS

Macmurray, John: The Structure of Religious Experience. New Haven: Yale University Press. (The Terry Lectures.) 1936. 77 pp. $1.50.

Roback, A. A.: Self-Consciousness Self-Treated. Cambridge: Sci-Art Publishers. 1936. 244 pp. $2.25.

THE JOURNAL OF SYMBOLIC LOGIC. Volume I, Number 2. Set-theoretic foundations for logic: *W. V. Quine.* Addendum to the article *Implication and deducibility: Arnold F. Emch.*

THE AUSTRALASIAN JOURNAL OF PSYCHOLOGY AND PHILOSOPHY. Vol. XIV, No. 2. Social Psychology: A Philosophical Analysis: *A. Boyce Gibson.* Law as a Psychological Phenomenon (I): *A. Patkin.* Psycho-Analysis and Aesthetics: *J. A. Passmore.* Determinism and its Ethical Implications: *A. M. Mardiros.*

REVUE PHILOSOPHIQUE. 61e Année, Nos. 7 et 8. Réflexions sur la théorie platonicienne de l'idée: *P. Lachièze-Rey.* "Sensibilité situative." L'apprenti-instinct: *H. Birnbaum.* Quelques aspects de la vérité et de la réalité dans la philosophie allemande contemporaine: *A. Stern.*

THE PERSONALIST. Autumn, 1936. The Source of Evolution: *R. T. Flewelling.* Day-dream and Life: *Sir John Adams.* Bergson and Greek Mysticism: *M. W. Hess.* The Ultra-Gothic Kant: *F. C. S. Schiller.* Personality, The Dynamic of Religion: *H. A. Youtz.* The Relation of Philosophy to Religion: *A. F. Liddell.* The Relation of Philosophy to Science: *Delo C. Grover.*

GIORNALE CRITICO DELLA FILOSOFIA ITALIANA. Anno XVII, Fasc. III. Il problema dell'oggettività (cont.): *I. Sciaky.* Vita e scritti di Erminio Juvalta (cont.): *A. Guzzo.*

LOGOS. Anno XIX, Fasc. 2. A proposito dell'Infinito nel pensiero dei Greci: *M. F. Sciacca.* Idealismo, realismo e filosofia della vita: *G. Mannarino.* La filosofia di E. Goblot: *R. Abbagnano Del Re.*

ZEITSCHRIFT FÜR DEUTSCHE KULTURPHILOSOPHIE. Band 2, Heft 3. Zur gegenwärtigen Lage der deutschen Universität: *Georg Dahm.* Die deutsche Philosophie im Zeitalter der Aufklärung: *Max Wundt.* Die ästhetische Problemstellung. Systematische Untersuchungen im Anschluss an ihre historische Entwicklung: *Hermann Glockner.*

REVUE NEOSCOLASTIQUE DE PHILOSOPHIE. Tome 39, No. 50. L'idée de nécessité en mathématiques: *F. Gonseth.* Du nécessaire en mathématiques: *M. L. Guérard des Lauriers.* Psychologie et Philosophie: *A. Michotte.* La psychologie expérimentale. Le problème de la conscience: *A. Fauville.*

RIVISTA DI FILOSOFIA NEO-SCHOLASTICA. Anno XXVIII, Fasc. III. Intorno al concetto di realtà: *Francesco Orestano.* Realtà ed esperienza: *Francesco Olgiati.* La Scuola neopositivista di Vienna: *Luigi Pelloux.* Problemi e ricerche di storia della filosofia: *Marino Gentile.*

BLÄTTER FÜR DEUTSCHE PHILOSOPHIE. 10. Band, Heft 1. Über den Grund der Notwendigkeit der Ehe: *Julius Ebbinghaus.* Grundsässliches zur Lebensphilosophie: *Philipp Lersch.* Luthers philosophische und geschichtstheologische Gedanken: *Peter Meinhold.*

NOTES AND NEWS

The Second International Thomist Congress will be held in Rome, November 23–28, 1936. The following topics will be discussed and the discussions will be led by the chairmen here indicated:

I. De cognitione humana, praesertim de criterio veritatis. Leo Noël et Franciscus Olgiati.

II. De quaestionibus quae philosophiae et scientiae communes exsistunt. Iacobus Maritain.

III. De constitutione corporum. Petrus Hoenen.

IV. De principiis vitae organicae. Victor Grégoire et Casimirus Kowalski.

V. De recentioribus inventis psychologiae experimentalis. Augustinus Gemelli et Emmanuel Barbado.

VI. De relationibus philosophiam inter et religionem. Reginaldus Garrigou-Lagrange.

Though latin is the official language of the Congress, communications in other languages are admitted. The Secretary of the Congress is Carolus Boyer, S. J. Communications and inquiries should be addressed to Congresso Tomistico Internazionale, Palazzo della Cancelleria Apostolica, Roma, Italia.